NATIONAL ACADEMY OF SCIENCES

FREDERICK SEITZ
1911–2008

A Biographical Memoir by
CHARLES P. SLICHTER

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Biographical Memoir

Courtesy of the Rockefeller Archive Center.

Fred Seitz

FREDERICK SEITZ

July 4, 1911–March 2, 2008

BY CHARLES P. SLICHTER

FREDERICK SEITZ WAS A BRILLIANT SCIENTIST. He was one of the founders of the field that became known as the physics of condensed matter; a wise and insightful leader of academic and scientific organizations; an influential spokesman for science nationally and internationally; a trusted counselor and adviser of many organizations. His contributions to the field of solid-state physics, to the National Academy of Sciences, and to The Rockefeller University were transformative. Ever alert, he used his influence to help many scientists at crucial stages of their careers. He died in New York on March 2, 2008.

I met Fred in 1949 when we both joined the faculty of the Department of Physics of the University of Illinois, he as research professor and I as a brand-new Ph.D. with the rank of instructor. Although he was only 38 years old, he was already a famous scientist. He had been elected a member of the American Philosophical Society in 1946, and he was elected a member of the National Academy of Sciences five years later. He was deeply and actively involved in solid-state physics. Fred had a profound effect on my scientific career. I take the liberty of telling a few of those personal aspects in this memoir. In preparing this memorial I have drawn on many of his writings, especially his 415-page autobiography *On* *the Frontier, My Life in Science.*[1] I have also been enlightened by a DVD recording of the memorial symposium in Fred's honor held at The Rockefeller University in February 2009, kindly made available by Fred's good friend and colleague Purnell Choppin. Ralph Simmons, Andy Granato, and Ned Goldwasser, three of Fred's colleagues at Illinois, have written a memorial article about Fred Seitz for *Physics Today*.[2]

THE EARLY YEARS

Fred was born in San Francisco. In his autobiography he writes, "The date was July 4, 1911, the year in which Rutherford discovered the atomic nucleus, Kamerlingh Onnes discovered superconductivity, and Sun Yat-sen overthrew the Chinese monarchy and established a republic." In this one sentence he captures many qualities his friends enjoyed about him: setting the event in a larger context, mentioning some history of science, and I suspect delivering the message with a twinkle in his eye since the reader was supposed to grasp that he was poking fun at himself by associating his birth with these great events.

He describes the neighborhood in which he grew up, with its mixture of ethnic groups and strong family traditions, with great warmth and affection. His father, after whom he was named, was born in 1876 in Germany. Owing to a family financial misfortune, his father was unable to go to college. Rather, he was apprenticed at age 14 to a Viennese-style pastry baker in Heidelberg for three years, and later for two more years in Innsbruck. At age 19 he came to America, settling initially in New York. Ten years later he was in San Francisco, where eventually he set up his own bakery. Fred's mother was born in San Francisco in 1883. She had a large extended family in the area. Although his father was serious and on occasion stern, Fred speaks of his parents as strong and loving.

Fred attended Lick-Wilmerding High School. The school had two curricular pathways, trade and college-bound. But during the first two years, all students took the same courses, including mechanical and freehand drawing, and shop (masonry, tinsmithing, and various sorts of woodworking). He writes, "While a program of this type was acceptable to West Coast colleges and universities, it would have been regarded as inadequate at one of the elite eastern private universities." He reports that it was held against him when he applied for graduate school at Princeton. He adds, "I believe that the continual downgrading of the status of hands-on technology of those institutions, with the admitted exception of computer use and programming, may provide additional signs of a form of national decay."

He goes on to write, "My experience in the school's auto repair shop taught me the satisfaction of technical competence, and brought some bonus rewards as well. One of my classmates Gene Mires had somewhere acquired a 1923 Buick touring car. We worked on it lovingly in the shop until it was in excellent condition, and in it a group of us went camping in Yosemite Valley for several weeks one summer." Throughout his life, Fred enjoyed the out-of-doors and nature as well as the company of others.

He speaks with special affection and admiration for his physics teacher, Ralph Britton, with whom he kept in contact right up to the time of Britton's death at age 96. The mathematics teachers were also clearly excellent. Graduating in the middle of his senior year, December 1928, Fred entered Stanford.

In his autobiography he describes in great detail and with warmth his years at Stanford. Because he arrived in January, he was out of phase with most of the freshman class that started in September. Thus, he writes, "most of my classmates had already formed bonds of friendship, and

I found myself somewhat detached from the group. As a result, I never developed much class spirit and went my own way, knowing that sooner or later I would have to decide what lead to follow." But he "treasured the great freedom to find one's own way that college life offered." He was warmly received by several members of the Physics Department. They included William Hansen (a member of the team of Bloch, Hansen, and Packard that in 1946 discovered nuclear magnetic resonance[3]) and John Clark. Edward Condon, a 28-year-old theorist who was a guest lecturer at Stanford in 1930, took a particular interest in Fred. He urged Fred to apply to Princeton for graduate school and played a key role in getting Fred admitted to Princeton.

Fred actually majored in mathematics at Stanford. That department, Fred reports, was a close happy family. In fact, when Fred left for graduate school, Professor Blichfeldt, the head of the Math Department, gave a small luncheon party for him. Fred's experience of having caring and attentive mentors was evident in his own style as a mentor of younger scientists. His caring touched many of us who later had the good fortune to know him.

Concluding that Caltech was "clearly the best technical school in the west," Fred decided that he should spend some time there. He transferred in the fall of 1930 for his junior year. The year proved exciting but had a drawback; there were no dormitory facilities and the only on-campus dining was a light lunch served in a primitive wooden shed. Of particular interest were lectures given by Linus Pauling, then in his twenties, about his quantum mechanical theory of the directionality of the chemical bond. It is interesting that in two years Fred would be working on the bonding of sodium atoms in sodium metal, developing a more rigorous approach characteristic of physicists. However, Fred reports that Condon once observed that chemists needed a theory that was applicable to the vast array of molecules of their world, and thus could not afford the luxury of dealing only with systems of great simplicity for which a high degree of rigor is feasible. It was quite characteristic of Fred that he did not scorn the less rigorous methods of the chemists but rather grasped and admired the creative power they gave chemists. He embraced both approaches.

Realizing that he could graduate in just one semester if he returned to Stanford, Fred returned there for the summer and fall of 1931, graduating in January 1932. Early in 1932 he took the train east to Princeton. Condon was there, finishing his famous book with George Shortley, *The Theory of Atomic Spectra*. Condon suggested to Seitz that a promising new area would be to use quantum mechanics to explore the properties of crystalline solids, trying to be as quantitative as possible. There was already important work of this sort for Fred to absorb from authors such as Peierls, Bethe, Frank, Bloch, Houston, and Van Vleck. When autumn 1932 arrived, Condon was so immersed in finishing his book that he suggested that Fred should work with Wigner, and helped arrange for that. Seitz had already absorbed Wigner's book on group theory, a knowledge that proved important for his thesis. So Seitz became Wigner's first American graduate student.

SOLID-STATE PHYSICS: FRED'S VISION

Wigner was a close friend of John von Neumann, dating from their teenage years in Budapest. Through Wigner, Fred got to know von Neumann. Thus began another friendship that was of great importance to Fred right up to von Neumann's untimely death in 1957. With Wigner, Fred began a study of solid materials, trying to understand the quantum mechanical nature of such properties as their cohesive energies and lattice constants. Wigner's original guiding thought for their work was that the main source of the cohesion energy of solids was a lowering of kinetic energy, on the supposition that the wave functions in a solid would be smoother than those of a free atom. They discovered that the Russian physicist Prokofiew had developed a core potential for the sodium atom that produced valence electron energies accurate to about 1 percent. All that fall they worked trying to demonstrate the correctness of Wigner's hypothesis.

Seitz reported that over the Christmas vacation he stayed in Princeton since the West Coast seemed so far away in those days before transcontinental air travel. The Physics Department was deserted. He then saw that they should perhaps focus on the Prokofiew core potential, and "after a happy week or so of integration by the method of finite differences and the use of a Monroe calculator, the cellular method of deriving solid-state wave functions was born, and when Wigner returned at the end of the Christmas holidays we carried on with increasing excitement. Our key paper[4] was published in May."

In a second paper[5] Wigner added a treatment of the electron correlation energy, making possible a full treatment of the cohesive energy of sodium. These papers applying symmetry principles to formulate a quantum theory of crystals opened the way for quantitative expansion of the field. They inspired in Fred a vision of how one could achieve an understanding of condensed matter in terms of quantum mechanics.

In the winter semester of 1933 a new student, John Bardeen, joined the Math Department at Princeton. His interests were in physics. On the very date he arrived he was introduced to Seitz. Bardeen became Wigner's second student, working on the physics of the surface layer. He published two[6,7] papers: *Theory of Work Functions of Monovalent Metals* and *Theory of Work Functions. II. Surface Double Layer.* Subsequently Hillard Huntington, Conyers Herring, and Roman

Smoluchowski joined the group of Wigner students studying physics of solids while Seitz was there. The discovery of the neutron and exciting developments in nuclear physics then drew Wigner away from study of solids to the field of nuclear physics, but in this one short burst Wigner had guided five of the most important theorists in the history of condensed matter physics.

In the autumn of 1934 Fred was invited to give a physics colloquium at Bryn Mawr College. There he met a young physicist, Elizabeth Katherine Marshall. They soon discovered they had many interests in common, including music. Betty had grown up in China where her parents were missionary teachers. After preparing for college in Shanghai, she attended Wilson College in Chambersburg, Pennsylvania. A year of graduate study at Cornell netted her an excellent fellowship at Bryn Mawr. Her two brothers, Lauristan and Robert, were both physicists. The friendship advanced rapidly with the result that Fred and Betty were married on May 18, 1935.

Fred received his Ph.D. in 1934 and stayed on for another year as a postdoctoral fellow. Then, in 1935 he joined the faculty at the University of Rochester as an instructor. The department head was Lee DuBridge, who had been recruited the year before from Washington University in St. Louis. In his autobiography Fred writes that the complete freedom for research and the light teaching load gave him "the opportunity to launch an ambitious project" that he had been contemplating since his student days: to write a comprehensive account of the various aspects of solid-state physics

> in order to give the field the kind of unity it deserved. This integration had been made possible by the development of quantum mechanics which offered the means of consolidation. The result, *The Modern Theory of Solids*, was published by McGraw-Hill five years later in 1940…Perhaps the greatest

value of the book was the attraction it provided for new, young investigators to undertake research in the area in the immediate post-war period.

I was one of those he lured.

In this monumental task Fred was greatly assisted by Betty. Fred writes in his autobiography, "I think it is safe to say that, in the writing of this book, Betty, who was soon deeply involved with the program, and I became familiar with every paper related to the field." I believe it is fair to say that this book[8] effectively defined the field of solid-state physics and played a major role in stimulating its advance.

While at Rochester, Fred already showed those qualities that characterized him: identifying younger scientists of promise, and doing his best to encourage them and help them in their endeavors. This quality was no doubt deeply rooted in his psyche, but I am sure his experience with particular faculty at Stanford, Caltech, and Princeton, about whom he speaks with great warmth in his autobiography, helped cultivate this quality in Fred. He mentions three undergraduates at Rochester whom he particularly enjoyed: Bob Dicke, Leroy Apker, and Joseph Platt. In the National Academy of Sciences memoir about Bob Dicke, a particular point is made of Fred's encouragement of Dicke, including abetting Dicke's transfer to Princeton in his junior year.

Fred writes that while working on his book

> it became evident that the rate at which the field of solid-state physics could be expected to evolve would be determined to a considerable degree by the number of experimental investigators attracted to it, and who might work in close association with the theoretical developments.

This vision provided the theme that occupied Fred for the next 30 years.

It led to his leaving Rochester after two years to join the General Electric Research Laboratory in Schenectady, New York. Fred was given a lab in the lighting research group headed by Saul Dushman. In 1938 in a paper on the plastic properties of solids Fred published a detailed theoretical interpretation of the experiments on thallium-doped alkali-halide crystals[9] in response to a paper from the laboratory of Robert Pohl. Awareness of these papers later led to some of his first invitations to aid industrial scientists after he joined the University of Pennsylvania.

While at GE, Fred and Betty purchased a plot of land on the shores of Lake George. The simple but tastefully designed cottage in a bay on the eastern side of the lake became over the years a "year-round refuge" for Fred and Betty, as well as later for their son Jack; Jack's wife, Elise; and their three children: Eric, Carey, and Jennifer.

Although Seitz particularly enjoyed the substantial freedom he had at GE, eventually he concluded that the Depression had produced a stagnation at the laboratory that stood in marked contrast with the situation at some research-oriented universities. He decided that GE was not developing a unified program but rather existed as splintered groups. Accordingly, in 1939 he accepted an offer from Gaylord Harnwell for an associate professorship at the University of Pennsylvania, with the opportunity to add members to the staff to create a team of experimental and theoretical physicists. Louis Ridenour, whom he had met as a summer fellow at GE, was also there. Seitz brought Andrew Lawson (a student of Shirley Quimby at Columbia University), Robert Maurer (a student of DuBridge at Rochester), James Koehler (a new Ph.D. theorist from Michigan), and Wigner's student Hillard Huntington. The group thus formed began an activity that Seitz guided, with some losses and some additions, culminating finally in the group he developed at the University of Illinois starting in 1949. The group of strongly cohesive theorists and experimenters was a tangible expression of the thinking that motivated Fred to write *The Modern Theory of Solids.*

In December 1941 the war broke out. By 1942 Harnwell was called away and Leonard Schiff was made the acting department head. Ridenour left for the Radiation Laboratory at MIT, where radar was under development. Fearing that neither Harnwell nor Ridenour would return to the department after the war, Seitz became uneasy about the future of his group. When the headship of the Physics Department at Carnegie Tech became open, Condon, who had moved from Princeton to the Westinghouse Research Lab in Pittsburgh, proposed Seitz for the position. Among the attractions at Carnegie Tech was a small research group under the leadership of Otto Stern, whose famous atomic beam experiment with Gerlach had demonstrated the spatial quantization of spin systems. The administration was supportive of Fred's outside interests and indeed felt that they might help others in the institution. So in late 1942 Seitz moved to Carnegie Tech and Fred and Betty moved to Pittsburgh. Huntington and Lawson joined the Radiation Laboratory at MIT. Koehler and Maurer joined Fred at Carnegie Tech.

Between 1939 and 1945 Seitz became heavily involved with applied research as the outside world learned of his abilities. As he says in his autobiography,

> Any hope I might have had of returning to a completely sheltered academic life when joining the University of Pennsylvania was a vain one. Not only would it have taken much more will power than I possessed to turn my back on scientifically interesting aspects of applied work, but I would also have to have had the good fortune to live in a world without wars. The more immediate requests, however, came from private industry and were a consequence of my published research at the General Electric Laboratory.

The first stimulus came from DuPont concerning the stability of dry pigments of various dyes. This interaction launched a 35-year association. Soon after the start of the war, scientists from the Research Laboratory of the Frankford Arsenal approached Fred. He helped develop a group of consultants that at various times included Tom Read, Hans Bethe, Cyril Smith, and William Deming. Fred also helped at the Naval Proving Ground at Dahlgren, Virginia, on the Potomac River. Lee DuBridge had left Rochester to head the Radiation Laboratory at MIT. In 1941 he contacted Seitz for help with the problem of crystal diodes that were used for frequency conversion of radar signals. Based on experimental results of the group led by Lawson and Park Miller, it became clear that controlling the purity of silicon or germanium was crucial. Fred enlisted the help of DuPont in making pure material. As the use in radar increased a group at MIT led by Henry Torrey orchestrated joint work by many other laboratories. This work, of course, laid the groundwork for the development of the transistor at Bell Laboratories after the war.

In autumn of 1943 Wigner asked Bardeen and Seitz to join a theoretical group at the Metallurgical Laboratory of the Manhattan District at the University of Chicago. Work was well along on water-cooled graphite reactors, and Wigner had become concerned about the effect of neutron bombardment on the integrity of the interior structures of the reactor. Bardeen was not able to accept, but Fred did. He enlisted Bob Maurer, whose experiments with Ed Creutz showed that the effects feared by Wigner were indeed serious. A program under Koehler was established at Carnegie to focus on the uranium slugs. In 1944-1945 the physics community at Carnegie Tech was thrilled to learn of the award of the 1943 Nobel Prize to Otto Stern (and the 1944 Nobel Prize to I. I. Rabi, who had studied molecular beam techniques in Stern's laboratory).

Seitz's last war activity took place in 1945. The secretary of defense, Henry F. Stimson, asked Fred to establish a small office at U.S. military headquarters in Europe to collect information on technical advances made by the Germans during the war that might be of particular interest to our military. In the process Seitz had interesting contacts with the Alsos group of "American" scientists who were in Europe to find out about the German activities concerning an atomic bomb.

During the winter of 1945-1946, Wigner went to Oak Ridge as director of the lab that had previously been run by the University of Chicago. Several of the staff from Chicago, including Alvin Weinberg joined him there. Wigner asked Seitz if he would take responsibility for a reactor education program. Fred agreed to do so for one year, taking a leave of absence from Carnegie Tech. In the fall of 1947 Wigner decided to return to Princeton. Alvin Weinberg was named as Wigner's successor, serving until 1974. In his memoir Seitz reports that although Wigner received the Nobel Prize for his work on the theory of the nucleus, Wigner felt that his greatest contribution to society was his work on the theory and technology of reactors.

SOLID-STATE PHYSICS: FRED'S YEARS AT ILLINOIS

Seitz returned to Pittsburgh from Oak Ridge fully expecting to remain there for the rest of his career. But in his autobiography he reports that it soon became evident that Creutz's program in nuclear physics was very successful and would require, in all fairness, almost all of any new appointments. In the fall of 1948 Louis Ridenour, who had become the dean of the Graduate School at the University of Illinois, invited Seitz to meet with him and Wheeler Loomis, head of the Illinois Physics Department. They offered Seitz a research professorship and the opportunity to make a number of departmental appointments. Seitz accepted in the winter of 1949. He brought with him from Carnegie Tech Bob Maurer, and Bob's recent Ph.D. student Dillon Mapother (to set up a low-temperature program), and recruited David Lazarus, a brand-new Ph.D. from the University of Chicago, who had been directed in his thesis research by Andy Lawson. As mentioned above, I also joined the faculty in 1949. The next year Jimmy Koehler followed from Carnegie. Thus, Fred had come close to reconstituting the team he assembled when he first went to the University of Pennsylvania.

Fred reports that the move to Urbana-Champaign was initially of some concern to Betty but that they promptly fell in love with the Midwest. Fred writes,

> The great American prairie has many qualities of the sea; there is the same kind of rich interplay between the sky and rolling land as is seen between sky and undulating water. Brilliant sunsets and magnificent storms hover over the vast spread of the land. And, like the sea, the land presents markedly different aspects at different times of day and at different seasons. In the early spring, the new plantings emerge as if by magic from the rich black soil. During the autumn, or in snow-covered winter, the pheasants move like gleaners through the grain fields, often whole-flocks at a time. In the summer the whole world seems bursting with life of all kinds...We never had a happier or more fulfilling time in our lives.

Indeed, Illinois provided the occasion for Fred to bring to stable fruition his dream of assembling a strongly interacting group of scientists, experimenters, and theorists.

Wheeler Loomis had gone to Harvard, been on the faculty of New York University, and then was brought to Illinois in 1929 to head the Physics Department. He was a close friend of I. I. Rabi and, indeed, had tried unsuccessfully to recruit him to Illinois. The Depression came shortly after Loomis started at Illinois, delaying the chance to build the department. It was followed by the war. Loomis was DuBridge's second in command at the Radiation Laboratory at MIT, which gave Loomis the opportunity to identify from firsthand experience many of the top young scientists, including my own Ph.D. thesis adviser Edward Purcell. Seitz remarks that he suspects that Loomis recruited Ridenour to Illinois, having met him at the Radiation Laboratory.

Loomis was a person of great strength. He radiated integrity and fairness. He thus created a sense of stability and peacefulness among the faculty. There were none of the tensions one sometimes hears about among university faculty. Wheeler and Edith Loomis had a large house about five blocks from the Physics Building. They rather frequently had large parties at their house, to which all of the members of the department were invited, as well as a number of faculty members from other departments in the university. Wheeler was a master preparer of the martini cocktail.

I found the department to be a very happy place. Even though I initially had merely the rank of instructor, the senior faculty welcomed me with great warmth. Before the arrival of Seitz, the department was almost entirely concerned with nuclear physics. There was a small cyclotron, a small betatron, and a much bigger one under construction under the guidance of Donald Kerst, inventor of the betatron. There were also people studying artificial radioactivity, the most prominent being Maurice and Gertrude Goldhaber. The warmth of the welcome accorded the new solid-state faculty is all the more remarkable when one realizes that the department was quite crowded. The new additions were shoehorned in a building that was already rather full. For example, when John Bardeen came in 1951, he had to share his office with his postdoc. An extra floor was inserted in a portion of the space between the third and fourth floors to provide offices for the theory grad students, such as John's student Bob Schrieffer. The students named it the Center for Retarded Study.

This spirit of welcome and congeniality that we all knew was created by Wheeler and Edith Loomis. The spirit lives on at Illinois today, and is a deeply revered tradition. No one could have been more at home with it or stronger at preserving it than Fred and Betty Seitz, since they had been practicing this style long before they came to Illinois.

Maurer's first Illinois student was Charlie Bean, who later made important discoveries about superconductors at GE. Koehler's first Illinois student was Ralph Simmons, a Rhodes scholar, whose Ph.D. thesis set a new standard for measurements of defects in metals and who later served as department head for years. Mapother set up a Collins helium liquefier and began developing low-temperature methods. Bob Hill, a nuclear physicist, took advantage of the new facility to investigate orienting radioactive nuclei at low temperatures. John Wheatley came in the mid-1950s to be a postdoc to assist Hill. Of course, Wheatley stayed on, establishing his own low-temperature group. His student Ansel Anderson later became head of the Illinois Physics Department. David Lazarus, who had been a student of Andrew Lawson, established a program investigating diffusion in solids.

In 1951 Fred learned that John Bardeen was unhappy with his situation at Bell Labs. With the assistance of Wheeler Loomis and William Everitt, dean of engineering, Illinois was able to lure Bardeen with a joint appointment in the Physics Department and the Department of Electrical Engineering. Bardeen soon brought other postdocs, including David Pines, Elihu Abrahams, and Leon Cooper. And, of course, Bob Schrieffer came to do a Ph.D. thesis with John. In 1957 Bardeen, Cooper, and Schrieffer (BCS) discovered their famous theory explaining superconductivity.

Hans Frauenfelder joined the Physics Department in 1952 as a research associate working with professors Jim Allen and Chalmers Sherwin, famous for their work on the neutrino. For his Ph.D. thesis in Zürich, Hans had employed nuclear physics techniques to study surfaces, and he soon set in motion a program in surface physics at Urbana. Andy

Granato was recruited from Brown University and set up studies using ultrasound.

In the early 1960s Leo Kadanoff and Gordon Baym came as assistant professors. Gordon is still at Illinois. Tony Leggett came as a postdoc. He went back to England for a few years, but we lured him back in 1983. He has told me that one of the attractions of Illinois was the experience he had while a postdoc there in the early 1960s. We had many other outstanding young scientists from abroad, attracted by the presence of Fred Seitz, including Fausto Fumi, Franco Bassani, and Gianfranco Chiarotti, all of whom later had distinguished careers in Italy, and Werner Känzig and Heine Gränicher from Switzerland, who likewise had distinguished careers.

The activity that Fred initiated and cultivated as soon as he finished his Ph.D. at Princeton reached a stable maturity at Illinois, bringing to full fruition Fred's dream of a community of theorists and experimenters who interacted strongly with one another, with scientists in the related fields of chemistry, metallurgy, ceramic engineering, and electrical engineering, and with scientists from all around the world.

Fred's efforts to keep a coherent life as a scientist at Illinois were under continual assault from people who recognized his wisdom, his wide knowledge of both science and technology, and his administrative skills. They realized that he received much stimulation and pleasure from tackling and solving new problems. In 1954 Fred was asked to serve as the chair of the Governing Board of the American Institute of Physics; he served for five years. Soon after, he was elected to the Council of the National Academy of Sciences, its governing board, for a three-year term. In 1959 he was asked to head an advisory committee to give scientific and technical advice to the United Aircraft Corporation. He was also enlisted as a scientific adviser to the Ampex Corporation.

In his autobiography Seitz reports that in 1954 John von Neumann, one of the commissioners of the Atomic Energy Commission, concluded that advances in solid-state physics research were as important for both science and technology as any field of science and that much more concentrated attention should be devoted to the field. My own suspicion is that this idea came from Fred, although he does not say so in his autobiography. Fred says that von Neumann had asked him "to prepare a proposal to the AEC for establishing such an interdisciplinary laboratory at the University of Illinois. He also urged other agencies of government to follow this lead. Von Neumann's death from cancer in 1957 derailed the initial proposal to the AEC, but the plan took root in the Department of Defense."

A number of universities applied for a laboratory. The bidding was opened and the individual in charge of selecting which institutions would be provided the new labs was Charles Yost, an old friend and supporter of the Illinois group. At first he felt that Illinois did not need such a lab because it was already so strong, but eventually he recognized that without a lab, Illinois might be raided by institutions that had labs and relented. Unfortunately, congressional politics stood in the way. Senator Dirksen of Illinois had angered a senator from Missouri by maneuvering to get a federal prison that was headed for Missouri to be placed instead in Illinois. In retaliation the Missouri senator succeeded in getting the lab for Illinois removed from the bill.

I remember vividly the disappointment all of us in Urbana felt when we learned that we would not be included. Don Stevens at the AEC finally found a solution involving the AEC, the Department of Defense, and the Illinois administration using funds from a state construction agency that were reimbursed over a 10-year period by the federal agencies.

Bob Maurer became the first director of the Illinois Materials Research Laboratory.

In 1955, realizing that the field of solid-state physics had grown vigorously since publication of his book in 1940, Fred decided to launch a series of books by active researchers to, so to speak, update his volume. With David Turnbull, a distinguished metallurgist, he launched a series with Academic Press: *Solid State Physics: Advances in Research and Applications.* Their original idea was a series of about six books, each about 400 pages in length, to be published over the next five years. The books turned out to be so successful that the series was extended to more volumes. This series, currently edited by Frans Spaepen, now has 61 volumes, the latest being published in 2009.

The materials research laboratories have played a very important role in the development of solid-state physics and materials research in the United States. I believe they should fairly be considered the final brick Fred Seitz put in place to complete the structure he set out to achieve when, with his fresh Ph.D., he began writing his book, *The Modern Theory of Solids.*

In 1959 when Wheeler Loomis retired as head of the Physics Department, Seitz was asked to become head and he accepted. Just as he was attempting to build a settled life as the department head, he was approached to spend a year in Europe as science adviser to NATO Secretary General Paul Henri Spaak. The position had first been filled by Norman Ramsey, who had agreed to hold it for a year. Fred likewise undertook the post for a year.

Then, at Illinois, Seitz was pulled still farther from an active life of science when he was asked in 1963 to become dean of the Graduate College and vice president for research. Gerald Almy succeeded Seitz as head of the Physics Department.

THE NATIONAL ACADEMY OF SCIENCES

In 1962 Fred Seitz was invited to stand for election for president of the National Academy of Sciences. The position had traditionally been a part-time position, but Fred's experience on the Council had alerted him to the fact that the duties of the presidency had grown, and he was not sure whether he could accommodate them with his duties at Illinois.

Three years later the Academy decided that the presidency of the Academy should become a full-time position. Seitz was asked to assume the position. He had some reservations. He and Betty had made long-term plans for life in Champaign-Urbana. Moreover, the Vietnam War was on and created a great deal of political controversy and animosity toward the Johnson Administration in many parts of the academic community. Recognizing that the Academy had a major role in advising the government, Fred realized that it might be difficult to reconcile this role with the sentiments of many Academy members. Indeed, that turned out to be the case. Fred accepted the nomination and was elected the first full-time president of the National Academy of Sciences for a six-year term beginning in July 1965.

Fred's term as president of the Academy set the organization firmly on a new course. Of the many actions he carried out as president I think of seven that were especially revealing of Fred's insight and touch:

1. Reorganization of the Academy membership structure
2. Formation of the National Academy of Engineering
3. Formation of the Institute of Medicine
4. Construction of the main auditorium and east wing of the Academy's building
5. Initiation of annual fundraising activities
6. Launching reports on the status of various fields of science
7. Formation of the Universities Research Association

In 1963 Seitz and Douglas Cornell of the Academy staff examined the trends in membership. They found that the composition of the 14 sections had changed markedly. For many years about 15 percent of the membership had a professional engineering education and a similar percentage had a medical education. The former were linked with the sections involving the physical sciences, the latter with those involved with life sciences, until separate sections in engineering and medicine were created. The rapid postwar growth of the basic sciences after 1945 led to a decrease in the fraction of the Academy elected in the engineering and medical areas. In response to this information the Council set up a committee to study the issue. It recommended establishing Classes of membership across the sectional groups and assigning new membership election quotas to each Class.

The role of engineering remained a subject of contention. Members of the U. S. Congress, such as George Miller of California, were sympathetic to the engineers' concerns. Realizing that the Congress might, if it became directly involved, "solve" the problem in a manner that could damage the Academy, Seitz enlisted Julius Stratton, president of MIT, and Eric Walker, president of Penn State, to assist the Council. It was agreed that a sister institution should be created under the Academy's charter. The National Academy of Engineering (NAE) was formed in 1964. The Institute of Medicine was created in 1970, under the NAS charter, to give a mechanism for balanced, authoritative medical advice.

Early in his term of office Fred decided that it would be helpful to various fields of science if from time to time they were to examine themselves in order to see where the most exciting future opportunities might lie. The first of these studies was led by George Pake, whom Fred had known when George had been an undergraduate at Carnegie Tech. *Physics:*

Survey and Outlook; Reports on the Subfields of Physics[10] was the result. Since then the Academy has made such studies for a number of fields of science from time to time.

Soon after taking office, President Kennedy appointed Glenn Seaborg as head of the Atomic Energy Commission. To acquire additional high-energy facilities Seaborg got authorization and funding for an accelerator to be built at the Lawrence Laboratory at Berkeley. High-energy physicists from other parts of the country were unhappy because prior experience convinced them that a facility located at Berkeley would not be welcoming to non-Berkeley scientists. A number of physicists came to Seitz about their concerns. In the autumn of 1964 Seitz met informally with the governing board of the Brookhaven National Laboratory, which had been organized after the war to promote nuclear energy and provide facilities for high-energy physics by nine eastern universities. It was managed cooperatively through an organization called the Associated Universities Incorporated. Seitz proposed that they extend their membership nationally to manage a new facility. They decided not to undertake such an extension. Seitz then convened a group of about 25 universities and solicited their views. They recommended forming a consortium to manage the next accelerator. They also proposed that a reevaluation of the design was in order. Seitz went to Seaborg to persuade him to agree, which fortunately he did.

The new organization was named the Universities Research Association. The Academy served as its initial base and helped in the organizing process and development of a board of directors based on regional representation. Norman Ramsey was chosen as its first president and R. R. Wilson of Cornell was selected as its first director. Responsibility for site selection was delegated to the Academy, which set up a committee chaired by Manny Piore. The committee worked out a set of criteria and then all interested states were invited to submit proposals. President Johnson requested that the committee present him with a short list of the six most promising sites. He selected Illinois. The laboratory, built under the direction of Robert R. Wilson with Ned Goldwasser as deputy director, was named the National Accelerator Laboratory. (It was renamed Fermi National Accelerator Laboratory in 1974.)

Detlev Bronk, Fred's predecessor as Academy president, had successfully raised money to complete the two wings of the original design of the Academy building. Soon after Fred became a full time president in 1965, he decided to try to raise funds to construct the auditorium that would complete the design. The campaign was successful, and an architect was selected just before Fred left the Academy.

In 1967 Detlev Bronk decided he wanted to retire as president of Rockefeller University. Fred had been chair of the Board of Trustees of the Rockefeller Foundation and a member of the board of The Rockefeller University. It was therefore very natural that the trustees asked Fred to be the new president of The Rockefeller University. Fred spent the academy year 1968-1969 commuting between the NAS and The Rockefeller University and assumed full-time service at Rockefeller in 1969.

THE ROCKEFELLER UNIVERSITY

The Rockefeller University began as a research institute. Founded in 1901 by John D. Rockefeller, it had thrived under the continuing attention of the Rockefeller family. In 1910 a research clinic was added to the institute. When Seitz became president, David Rockefeller was chair of the governing board. Bronk had made a number of significant changes during his tenure, including adding small departments of physics, mathematics, and philosophy, selecting the faculty with great care. He founded a small graduate school with about 100 carefully selected students. He had also added a number of amenities, including a new faculty center and a student dorm. He added a laboratory building. All of these things together with the growth of the research activities and changes in federal funding produced a growing deficit for the institute.

Fred had his own style of administrative organization that was more formal than that of Bronk. Consequently he brought in several new people to help him in his presidential responsibilities. From the University of Rochester he brought Al Gold, one of his former postdocs at Illinois, as vice president for academic resources. Gold brought David J. Lyons, also from Rochester, as vice president for business and finance as well as treasurer. A key appointment was Rod Nichols, another physicist, with whom Fred had worked when he was chair of the Defense Science Board and Nichols was on the staff of the deputy director of defense research and engineering in the Office the Secretary of Defense. He joined Fred in 1970 as vice president, later becoming executive vice president.

In 1970 the decision was made to limit the number of graduate students to about 100. In conjunction with Alick Bearn, physician-in-chief of the New York Hospital of the Cornell University Medical College, Rockefeller began a program to give some of the students the chance to obtain an M.D. degree from Cornell while also obtaining a Ph.D. from their research program at Rockefeller. Although Seitz fully appreciated the magnificent advances in cellular and molecular biology that took place in the 1950s and 1960s, he also realized that the practice of medicine "is a human art, based on wisdom, skill, and experience…Therefore it is vitally important to maintain links between the research scientist and the practitioner, and the University Hospital at Rockefeller had performed that function with distinction." Fred likened the relationship between clinical work and basic biological research to the relationship between engineering and applied science to basic science in his own field of solid-state physics, where he was a strong proponent and practitioner of a robust linkage.

Fred felt that as president, Detlev Bronk had been ambivalent toward the hospital, feeling that a great revolution in medical care was in the offing. However, Bronk had appointed Maclyn McCarty as physician-in-chief of the hospital. Together with Oswald T. Avery and Colin McLeod, McCarty had demonstrated that DNA carries the genetic message. Thus he had given the hospital strong leadership in the clinical area. Seitz and McCarty developed a close working relationship on this and many other matters. When McCarty retired, Fred appointed Attalah Kappas to maintain the strong role of the hospital and clinical research.

All of these activities placed strong pressure on an already severely strained budget. Although the staff had been augmenting the internal funding by applying for government grants, the financial pressure was great. Therefore, Fred proposed that a formal program was needed to seek private funds. A special characteristic of these efforts was to have the prospective donor meet with one or more members of the scientific staff. This practice led to many close links between the scientists and the donors, yielding benefits beyond the simple monetary assistance. Fred created an advisory council of some of these benefactors that met on campus several times a year.

In his autobiography Fred writes, "No private endowment is so great these days that one can ignore federal funds. To ignore their availability would be to abuse the special flexibility provided by limited private funds, which usually have fewer strings attached to them at present." In this statement I suspect one hears echoes of some strained conversations he had with staff resulting from the transition from the early period where the endowment supported a larger fraction of the research. A byproduct of the fundraising effort was the appointment of Pat Haggerty, chief executive officer of Texas Instruments, as chairman of the Rockefeller board and Fred Seitz to the board of TI. Fred clearly enjoyed the activity at TI, especially the continuing contact with the heads of the Research Laboratory, Ross MacDonald and then Norman Einspruch.

In his autobiography Fred writes,

> With all due respect to many other attractions, it is my opinion that the single most impressive feature of Rockefeller University has always been the exceptional quality and character of its senior staff, and of the young scientists who work with them.

He quotes F. Peyton Rous: "It is not a place where one feels compelled to do anything trivial." Fred points out that "for senior staff, routine work is minimal. There are no formal classes and few committees. In fact, the most onerous task they currently face is probably the need to prepare competitive proposals for federal agencies—a fact of life in a democratic society."

This strong statement expressing Fred's assessment of Rockefeller University shows very simply, in my opinion, why he abandoned his earlier intention to return to Illinois at the end of his term as president of the National Academy of Sciences to undertake the presidency of Rockefeller University.

When Seitz assumed the presidency in 1968 at age 57, Rockefeller University had a policy specifying that the president should retire at age 65. His actual retirement occurred in 1978, when he was a mere 67 years old. His next 30 years were full of vigor and creative activity.

"RETIREMENT"

After retiring from the presidency of Rockefeller University in 1978, Fred and Betty were fortunate to be invited to stay on in an apartment owned by Rockefeller University. Kappas provided him with a suite of offices in the University Hospital. These arrangements enabled Fred to keep up a very active life. He joined the boards of the Ogden Corporation and the Lounsbery Foundation. He also joined the board of the China Foundation, based in Taiwan. When Pat Haggerty formed an advisory group to the Taiwanese government, named the Science and Technology Advisory Board, Seitz became its vice chairman. He also joined a research advisory committee to help the R. J. Reynolds Company make medical grants to universities. That committee included Maclyn McCarty and James Shannon. One of their most successful contributions was funding the research of Stanley Prusiner, who was striving to understand the origin of scrapie, a neurological disease in sheep. The support at a time when Prusiner was about to lose his job and with it his research opportunity was described in exciting terms by Prusiner at the memorial symposium honoring Seitz at Rockefeller University in 2009. The work led to the discovery of prions for which Prusiner was awarded the Nobel Prize in 1997. He invited Fred and Maclyn McCarty to the Nobel award ceremony.

Fred's other activities included participating in a number of scientific panels. With Dean Eastman of IBM he headed an NAS panel to study the needs to update experimental facilities for materials research in the United States. With Bob Richardson of Cornell he headed a study concerning needs for facilities providing high magnetic fields.

When President Reagan announced his plan to support the Strategic Defense Initiative, the President's science adviser, George Keyworth, asked Seitz to chair an advisory committee to undertake critical reviews of the program and its development. Although many in the scientific community were strongly opposed to this program, Seitz wrote that

> early on, some proponents of the program spoke of developing an "impenetrable shield" which clearly would be very difficult to maintain if hundreds of attacking missiles had to be dealt with in a short period of time. The more sober and realistic view taken by our advisory group was that we could hope to achieve a sufficiently high level of defense as to severely limit the effectiveness of a first strike by the Soviets while making U.S. retaliation a virtual certainty.

Fred seriously questioned the scientific basis for global warming estimates. In a 1991 article with W. Nierenberg and R. Jastrow[11] he outlined his concerns at that time. His continuing, outspoken skepticism and some actions to enlist support for his position on this highly charged issue beclouded his image in the eyes of some colleagues.

The happy years of retirement were interrupted sadly with the discovery that Betty was suffering from cancer. From the start, after their lightning fast courtship, Fred and Betty were a team even, as mentioned above, involving the writing of Fred's famous book. They maintained close contact with Betty's family. When they were at Illinois, Betty's brother Larry Marshall was based in Indianapolis, Indiana, a two-hour drive from Champaign. When Fred and Betty arrived at Illinois, we all soon got to know Larry and his wife, Lucie.

I remember vividly dinners and larger parties at the Seitzes' house in Urbana when we had some visiting scientist of interest to Fred. Betty was somewhat shy, but she was a warm and gracious hostess. In Urbana she took up the piano again and enjoyed an association with other musicians. From the start Fred and Betty shared a love for music. She lent important support to all their joint activities. She was warmly remembered at the memorial symposium held at Rockefeller University in 2009. Several of the speakers showed delightful photos of Fred and Betty together, including some showing them riding on the special rail system that Fred had installed at Lake George to help transport groceries and other things from the parking lot high above the lake to the cottage by the lakeshore. Betty died in 1992 after a long illness.

After Betty's death, Fred embarked on a series of books and articles about the history of science. Rod Nichols in his remarks at the memorial symposium said, "At heart, Fred was a physicist." He had an insatiable curiosity, a deep interest in all branches of science, and a love of history. The books he wrote in retirement were a method for him to celebrate science and the people who did science, especially physics and its applications. These writings brought him back to physics. He sought to bring to life the human role in the creation of science. These accounts express his belief in the importance of basic research motivated by the curiosity of the investigator as the source of the great discoveries. They also express his strong belief in the importance of the coupling of science to technology and his respect for importance of work on applications of science.

In 1973 while at Rockefeller he and Rod Nichols published their book *Research and Development and the Prospects for International Security*,[12] laying out their belief that strong support for research in basic science and in development were the vital underpinnings to international security. In 1992 Fred published *The Science Matrix, the Journey, Travails, Triumphs.*[13] In 1994 he published his autobiography *On the Frontier, My Life in Science.*[1] In 1996 he published *Stalin's Captive, Nicholaus Riehl and the Soviet Race for the Bomb.*[14] This remarkable book is in part Seitz's translation into English from the German of Riehl's book *Ten Years in a Golden Cage.* Written by Riehl in 1955 but not published until 1988, it tells of Riehl's work on the production of pure uranium for the Soviet atom bomb. The first 60 pages by Seitz, titled "The Backdrop," set the stage for Riehl's book. Fred's book is a scholarly work of the first magnitude, as becomes immediately evident from Fred's description in the preface of the research process that enabled him to write the Backdrop and do the translation.

As I have remarked above, Fred was elected to the American Philosophical Society in the spring of 1946 at the remarkably young age of 34. (He in fact holds the record for longest membership in the history of the society, 62 years, beating out the runner-up James Madison, the fourth President of the United States, who was a member for 51 years). Fred regularly attended the meetings and contributed many articles to the Proceedings as described below. I looked forward every year to seeing him there.

Together with Norman Einspruch, Fred wrote *Electronic Genie, the Tangled History of Silicon,*[15] published in 1998. Fred wrote a first version of this material for the journal *Physics Today.*[16] He published a second version in the *Proceedings of the American Philosophical Society* in 1998.[18]

In the opening paragraph of *Electronic Genie* he wrote,

> The so-called information or computer superhighway is paved with chips of silicon. This is a triumph of advances in understanding the solid-state or materials science. It is also a product of the knowledge gained in the convergence of major areas of chemistry, metallurgy, and physics, particularly those related to the behavior of solids in the presence of electric and magnetic fields, when applied to the design of electric circuits.

Both Fred and Norman, as major contributors to that knowledge, were singularly well qualified to tell the story.

Fred also wrote other articles that appeared in the *Proceedings of the American Philosophical Society.* In 1999 he wrote *The cosmic inventor, Reginald Aubrey Fessenden (1866-1932)*[17] honoring Saul Dushman, his friend and adviser from his days at Schenectady. Fessenden was a prolific inventor who made important contributions to radio (inventor of amplitude modulation), among many things. As in all such writings Fred provided a three-dimensional portrait of the man, his upbringing, and his family, as well as explained the scientific discoveries and their significance.

In *James Clerk Maxwell (1831-1879) member APS 1875*[19] published in 2001 Fred gave an interesting picture of what Maxwell was like as a person in addition to an historical account of his discoveries. It includes an interesting account of the relationship of Maxwell's equations describing electric and magnetic fields to Einstein's special theory of relativity, including the discoveries of Lorentz and Poincaré.

In 2002 Fred published *China and natural science: Conundrum. In remembrance of Ta You Wu (1907-2000)*[20] about the life and accomplishments of Professor Wu, with a discussion of why it was that China had not developed modern science long before it developed in the West. Preparation of the article drew on an article on this topic written by Professor Wu. Included is a touching note written by C. N. Yang to his teacher, Professor Wu, on the occasion of the award of the Nobel Prize to Professor Yang.

In *Henry Cavendish: The catalyst for the chemical evolution*[21] in 2004 Fred brought out the importance of the chemical studies of Cavendish to the later work of Lavoisier.

Reading Fred's autobiography, one sees readily how much Fred enjoyed people. He writes warmly of his boyhood friends in San Francisco. He met an astonishing array of people from all over the world and clearly enjoyed this aspect of his life. He had the ability to view the human foibles he encountered with an air of amused detachment. Indeed, Fred had a superb sense of humor, but he expressed it usually in a quiet, subtle manner that required the listener to be alert to spot the fun. In reading Fred's autobiography, those who knew Fred can find many places where one senses that the words were written with a twinkle in Fred's eye. He had great self-control, even under circumstances that must have been highly aggravating. "Well, we'll just have to pull up our socks" was an expression with which he was known to respond when frustrated in an endeavor or when reaching a dead end in some effort.

Fred was chair of the board of the American Institute of Physics from 1954 to 1959, president of the American Physical Society in 1961, a member of the President's Scientific Advisory Committee from 1962 to 1967, and chair of the Defense Science Board from 1964 to 1968.

He received many honors in addition to election as a member of the National Academy of Sciences (in 1951) and the American Philosophical Society and countless honorary degrees. Several prizes or medals of particular significance that Fred received are the Franklin Medal of the Franklin Institute (1965), the National Medal of Science (1973), the Compton Medal of the American Institute of Physics (1970), and the Vannevar Bush Prize of the National Science Foundation (1983). The University of Illinois named its Materials Research Laboratory after Fred in 1993.

SOME PERSONAL OBSERVATIONS

Fred Seitz has influenced the lives of many people. In this final section I give a few examples from my personal experience. Fred undertook the writing of his famous book *The Modern Theory of Solids* to influence the field of solid-state physics. It strongly influenced me when I was finishing my own graduate work at Harvard in 1949.

I did a Ph.D. thesis with Edward Purcell in the field of magnetic resonance that Purcell, Robert Pound, and Henry Torrey had just discovered[22] (January 1946). For my thesis I studied the electron spin resonance of paramagnetic salts. In the fall of 1948 I met Wheeler Loomis when he came to Harvard on a recruiting trip, no doubt in part to see if Purcell had some student in this new field. Loomis invited me to visit Illinois. I had decided that magnetic resonance would be a powerful tool to study the properties of solids, but though I knew of Fred's book, I had never taken a course in the field. When I got to Urbana, Loomis showed me the department and then offered me a position as instructor when I completed my Ph.D. I had heard a rumor that Seitz was about to move to Illinois and asked Loomis if that were true. He replied that Seitz had been offered a position but had not yet given his reply. I asked Loomis if I could wait to give my reply until Seitz gave his answer. What colossal nerve on my part! Loomis said yes, I could wait. Several weeks later Loomis called to say that Seitz had accepted, and I immediately accepted. Since I was offered a position because I was Purcell's student, I was not recruited by Seitz and not officially a member of the solid-state group that he had recruited. But Seitz rapidly made me feel welcome.

It was not long before I began to experience the benefits of Fred's presence. My first student, Dick Norberg, took Fred's course on solid-state physics in the fall of 1949. After hearing Fred lecture about the interesting effects hydrogen had when introduced into the lattice of metallic palladium, Dick proposed that for a thesis he study the hydrogen nuclear magnetic resonance (NMR) of that system. So Fred was responsible for the thesis topic of my first Ph.D. student. Dick later went to the Physics Department at Washington University where he did magnetic resonance work of exceptional importance, receiving the triennial prize of the International Society of Magnetic Resonance. For many years he was their department head.

Fred was a strong believer in the value of getting postdocs who came from other laboratories or universities. In 1951 Al Overhauser got his Ph.D. at Berkeley, where Charles Kittel was his thesis adviser. For his thesis Al calculated the spin-lattice relaxation time of conduction electrons. Al has told me how Kittel said to him, "Now we need to get you a job."

He sent Al out of his office for a few minutes, then called him back in, saying, "Would you be willing to go to the Midwest? I have just talked to Fred Seitz and he has offered you a postdoctoral position working on radiation damage at Urbana." That was what it was like to deal with Fred Seitz.

So Al came to Urbana in the fall of 1951. My students and I soon got to know him. One day, probably in late 1951 or early 1952, Al heard Dick Norberg give a talk about his thesis. Al has told me that this stimulated him to look again at his own thesis, and within two days he had come up with his idea for dynamic polarization of nuclei—the famous Overhauser effect. Within a year my student Tom Carver and I had demonstrated the effect in lithium and sodium metals. The magnetic resonance community was agog at Overhauser's idea. In 1954 Tom went to Princeton as an instructor. He had a distinguished career at Princeton, but tragically he died at a young age in 1981.

I had another especially talented student at exactly that time, Don Holcomb. He and Dick Norberg had been studying the alkali metals by NMR, seeing such things as the ability of NMR to reveal self-diffusion in the solid state and how the NMR signal gave detailed information about the properties of the conduction electrons. Fred played a crucial role in getting Don his post-Ph.D. job.

In the spring of 1954 Lloyd Smith, head of the Cornell Physics Department, and Fred were riding on the subway in New York. Smith asked Fred if he knew of any good students at Illinois. Fred immediately told him about Don. Smith invited Don to visit Cornell and hired him. Don had a long and distinguished career at Cornell, serving as chair of the Physics Department on several occasions, as director of their Laboratory of Solid State Physics, and as president of the American Association of Physics Teachers. I found it amazing that Fred knew about my students and what they were doing, and about their quality. But this is what Fred was like. Also typical was his immediate grasping of the opportunity to help the student (as well as the student's adviser.)

These stories illustrate how concretely and immediately I was a beneficiary of being near Fred Seitz. I can give further insight into how alert and skillful Fred was in helping young scientists by the following story. In 1949 most of the research in nuclear physics at Illinois was supported by a large grant from the Office of Naval Research (the ONR). Wheeler Loomis was the principal investigator. This was the source of my research support initially. I spent almost zero time applying for money, merely giving Loomis a couple of pages every year describing what my students and I had done the year before. Loomis dealt with the federal agencies. In 1959 Fred came to me one day to say that the ONR grant was growing successively more and more financially pinched. Since I was not really doing conventional nuclear physics, and since he said I was by now well established in solid-state physics, he suggested that I should try to get research support of my own.

I had never previously applied for a research grant. I began by trying to find out where to go for money, how one went about the process, and so forth. Then a few days later Fred came to me and said, "I have just been to Washington and saw Don Stevens at the AEC. I told him that you were looking for research support. He said that he would be glad to provide it and that you should send him a letter telling what you needed." Thus began my support from the AEC. They supported me for many years as the agency morphed into its present form, the Department of Energy.[23]

Like many of my colleagues at the University of Illinois, my contacts with Fred remained warm over the rest of his life. He made the effort to keep connected even as his career drew him to Washington and New York. It was always a special joy for me to hear from him or to see him.

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